

Letters to the Editor.

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Metallic Coloration of Chrysalids.

THE chrysalids of many butterflies have the appearance of being gilded. The area covered by the gilding is variable even in the produce of the same batch of eggs, in some cases showing only round the prominences on the back, and in others diffused over the greater part of the surface.

Modern naturalists seem inclined to explain animal coloration as being either protective, warning, mimetic, or the result of sexual selection, but the ornamental gilding above referred to can scarcely fall into any of these classes. Protective resemblance, *i.e.* the similarity between an animal and its usual surroundings, is met with everywhere, and natural selection would, or might, act continuously, to promote this resemblance. The opportunities, however, for selection to cause members of different genera to assume the same appearance would be much fewer.

I do not know of any evidence which would point to the models as being of older types than the supposed copyists, but even if such evidence existed, the probability that the similarities are due to selection is of a different, and lower, order than that which connects selection and congruity of colouring with environment.

Considering that, apart from the latter tendency, nothing whatever is known as to the origin of the various types of markings and colour patterns, it would seem justifiable to put down many cases, at least, of what is called mimicry to the action of the same unknown cause.

Chrysalids are often protectively coloured (amongst the Pieridæ, for instance, if the pupa is placed on a wall or paling it is greyish with dark specks, but if on a leaf the colour is a uniform green); there are very few positions, however, in which a bright metallic object would not be more conspicuous than a duller surface similar in tint to its background.

The nature of the metallic colours, of which there are such abundant instances in the animal world, has always been a matter of interest, and during the last twenty years I have examined many hundreds of examples of the kind, and have found in every case that the metallic appearance was due to some form of "interference."

Colour, in general, is the result of a selective action of matter on the light which reaches it, and may arise either from a relation between the wave period of the light and the molecular periods of the matter, or from a relation between the wave-lengths of the light and some structural dimension in the substance built up of numbers of molecules.

The first of these alternatives includes all pigmentary colours, while the action of the second is properly described as "interference."

Pigmentary colours are analogous to the phenomena of resonance in sound, and interference effects to those of combined echoes.

The interference colours of birds and insects take so many forms that it is not always easy, at first sight, to distinguish them from pigments, but a decisive test may be found by subjecting the coloured material to pressure.

If the application of pressure causes the colour to disappear, interference may be safely inferred. If

the colour changes, but does not vanish, interference is probable, but not certain.

Fig. 1 shows the apparatus which I made for carrying out this test. It consists of a quartz plate A and a plano-convex quartz lens B, which can be pressed into contact or withdrawn from A by the adjusting screw C. Other adjustments are provided for traversing A in its own plane and for clamping the apparatus in its correct position on the stage of the microscope.

In use, B is first brought into contact with A, and the series of Newton's rings thus formed is centred in the field of view. B is then withdrawn and a small piece of the material to be tested is placed on A, and A is moved by its own adjustment screws so as to bring the test-piece into the same position as was occupied by the centre of the ring system.

Various other tests, such as immersion in various fluids, may be applied to determine the character of the interference, and some of these are mentioned in the paper on "Iridescent Colours" (Proc. Roy. Soc., A, vol. 85, pp. 598 *et seq.*).

In the majority of the cases which I have examined the colours seem to be analogous to those of Lipmann

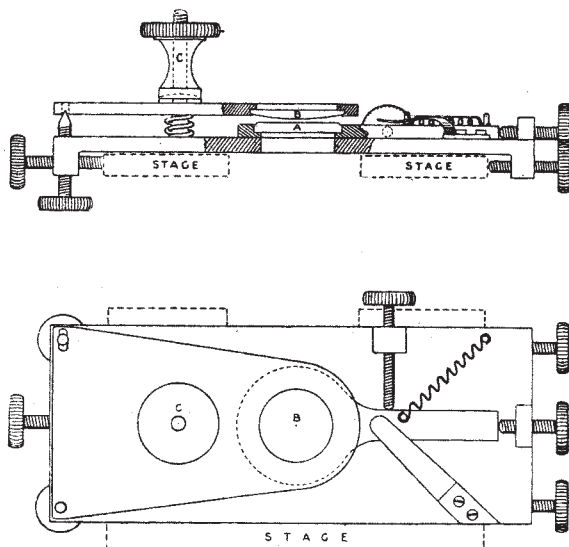


FIG. 1.—Section and plan of apparatus for applying pressure to coloured test-pieces.

films, but it is almost impossible to cut sections thin enough to show this by direct evidence. It may be stated, however, that in the case of birds and insects they are not due to diffraction (an explanation which has often been put forward in books on natural history). I have not yet examined the iridescent colours of fishes and reptiles. From their appearance I should expect that of the former to be interference phenomena, but that the splendid iridescence shown by the scales of some pythons may be the result of diffraction.

It is true that the scales of many Lepidoptera are traversed by fine and regularly spaced lines, and I have used a single scale from a *Morpho* as a diffraction grating, but the diffraction spectra have no relation to the colour which is directly reflected.

To return to the gold of the chrysalids, I find (1) that the gilding disappears completely when pressure is applied; (2) no immediate change occurs on immersion in water, alcohol, or xylol, but in the course of a few days the gold tends to become more orange; (3) the metallic appearance vanishes when

the specimens are dry, and this happens also (probably for the same reason) about a day before the imago escapes; (4) when the chrysalis is examined with a low power, the gold is seen to be made up of small patches of colour ranging from red to green, with yellow predominating; and (5) all these colours shift towards the blue as the angle of incidence increases.

An enlarged sketch from a photograph of a thin section of the wall of the chrysalis is shown in Fig. 2. Photographs of subjects such as these are generally unsatisfactory owing to the impossibility of getting all the significant features in focus at the same time.

The section shows an inner membrane (*a*), which stains readily, and is traversed by closely spaced fine lines (about 30,000 per in.), covered by a thin chitinous coat (*b*) having many corrugations on the outer side.

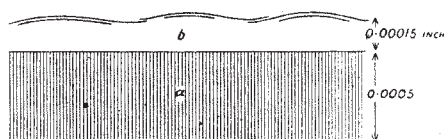


FIG. 2.—Section of a portion of a chrysalis of *V. urticae*.
(From a photograph.)

Of the foregoing observations, which were made on a series of chrysalids of *V. urticae*, (1) proves the colour to be the result of interference, and (5) is in agreement with this; (2) shows that the interference is not a surface effect, but is due to some internal structure in the chitin; and (2) and (3) together indicate that the periodic structure changes its properties and dimensions by wetting and drying.

In some very thin sections I have thought I could recognise separate layers at the borders of the chitinous coat, but it is difficult to distinguish with certainty this apparent stratification from the diffraction bands which are often seen at the boundary between two substances of different refractive index.

A. MALLOCK.

9 Baring Crescent, Exeter.

Sex-manifestations and Motion in Molluscs.

IN NATURE of October 13, p. 212, Mr. G. C. Robson discusses the application to molluscs of the present writer's suggestion that bisexuality in animals may be a direct physical result of a freely moving habit of life, and that hermaphroditism may be a direct physical result of a sedentary or sluggish mode of life. It was also suggested that it follows from this hypothesis that all sedentary or sluggish animals may be suspected of hermaphroditism where they are now supposed to be bisexual, especially in view of the fact that sex-change may be obscured by a rapid change-over of sex-characters such as we know takes place in the oyster.

Mr. Robson's discussion brings out some of the difficulties experienced in applying the generalisation to molluscs, but at the same time illustrates some facts in its favour. It is suggested that the marine Euthyneura, which are hermaphrodite, are as "active" as the Streptoneura, which, except for some sedentary and parasitic forms, are bisexual. This difficulty is a real one if a comparison be made merely of the behaviour of the animals in captivity; but if "activity" be defined in relation to muscular development, it will at once be seen that there is a great difference between the Streptoneura and the marine Euthyneura. An *Aplysia*, or even a *Scaphander*, as

a type of marine Euthyneura is a flabby and feeble animal in comparison with the strong, powerful, muscular Buccinum as an equivalent type of Streptoneura. Indeed, the difference in muscularity between the two groups might very well be regarded as of fundamental importance by a biophysicist.

It is considered undesirable to attempt to define closely such terms as "sluggish" and "active," since if there is any underlying truth in the hypothesis proposed, definitions would soon begin to crystallise out when the problem begins to be seriously attacked. The degree of muscular development will, however, probably enter largely into the definition of "activity" from this point of view. In this respect the following quotation—warm from the press—"Mechanism of Life," by J. Johnstone, 1921, p. 218) is of great interest:—"The animal¹ is characteristically a machine for the conversion of potential chemical energy into movement of body and limbs, and the movement inevitably leads to friction." As an illustration of another apparent contradiction of the hypothesis, the Ctenophora may be mentioned. These forms (excepting the Beroë group) are "active"—that is, they move about quickly; but there are also at least many of them hermaphrodite. The Ctenophores (except the Beroë group), however, move about, not by means of muscular movements, but by the rhythmic action of compound flagella, and this, it is submitted, may be regarded from a biophysical point of view as a mode of locomotion fundamentally different from that brought about by muscular action.

The illustration from Mr. Robson's letter in favour of the hypothesis relates to the fact that among the Streptoneura, which are typically bisexual, are a few sedentary and parasitic forms. Now these parasitic forms are hermaphrodite, and so also are those sedentary forms, namely, *Crepidula* and *Calyptæa*, which have been critically examined. Moreover, it may be observed that such simple cases of hermaphroditism as occur in *Crepidula* and *Calyptæa* have been described only recently, and that, therefore, more difficult cases may very easily have been overlooked; further, it is not at all incompatible with possible hermaphroditism in a species for a hundred specimens of that species (not to mention a mixture of several species) to be examined unsuccessfully for products of both sexes; even in the oyster Hoek ("Rapport over de oorzaken van den achteruitgang in Hoedanigheid van de Zeeuwsche oester," p. 174, Uitgegeven door het Ministerie van Waterstaat Handel en Nijverheid, s' Gravenhage, 1902) found only 11 hermaphrodite forms out of 130 examined microscopically, and we now know that sex-change occurs seasonally in this species. In other species—where sex-change may occur only once in a lifetime—it would be easy to predict a case where less than 1 per cent. of a random sample would be hermaphrodite, especially as both sex-products may not be produced simultaneously.

It is not a difficult matter to raise objections to the general hypothesis in relation to doubtfully active animals; therefore, results of practical applications of the suggestion are more interesting. For instance, are all the sedentary Streptoneura, the Scaphopoda, and the Polyplacophora really hermaphrodite, although they appear to be bisexual? It must be remembered that Nature can attain the same end in all kinds of ingenious ways, as though delighting in obscuring the application of any underlying principles, if such do, indeed, really exist. For example, the present writer applied the hypothesis stated above to an inves-

¹ Prof. Johnstone is of course referring to the metabolism of undoubtedly active animals.